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THE NEEDS OF RESEARCH[1]

THE occasion which brings us together to-day is one of profound moment alike to biologists and to the devotees of other branches of science; for in the dedication of your new laboratory we make distinct and formal recognition at once of our existence in a universe chiefly unknown to us and of the most effective method thus far devised for interpreting it. This universe is the complex of phenomena in which we find ourselves and of which we humans form a part, and this method is the method of research. The evolution of our race may be summed up under the two heads of man's relations to and of his means of investigating this complex of phenomena in which he plays the rôle usually of a nameless supernumerary, but occasionally also the rôle of interpreter, or even manager, in an ephemeral presentation of some aspects of the larger drama of life. The event we celebrate, therefore, should stimulate our keenest philosophic interest and rouse our enthusiastic admiration for the favoring circumstances which have made it possible to secure this substantial adjunct to the rare opportunities which have long made Woods Hole a resort for students and investigators in biological science. This event means progress; it marks a definite step in advance along lines of proved advantage to society at large; and it makes additional steps forward easier not only for your organization, but for all similar organizations. Moreover, the age in which we live is preeminently an age of restless, if not

[1] Address read on the occasion of the dedication of the Marine Biological Laboratory, Woods Hole, Massachusetts, July 10, 1914.

impetuous, enquiry, and the development of establishments through which research may be pursued patiently and systematically to demonstrable conclusions is one of the most inspiring signs of our times as well as one of the most essential agencies for the conservation of the best interests of community and national life.

For without the aid of such establishments it is not evident how we may distinguish what is fundamental and advantageous in our advancing evolution from what is accidental or inimical to it. Many eminent minds, indeed, are appalled at the temerity and the impatience of the more radical members of contemporary society in their manifestations of the prevailing spirit of inquiry. Traditions, customs, cherished beliefs and legal methods of procedure are all being challenged. In an era of unparalleled enlightenment so far as available knowledge is concerned we are frequently startled by the fact that there are yet numerous localities where intellectual darkness, if not abysmal ignorance, prevails. In an era of unequaled philanthropy and international amity there are nevertheless instances of wars whose atrocities beggar description, while national armaments which threaten national bankruptcy go forward unimpeded. Although the administration of justice was never on the whole so equitable and so merciful as at present, we are becoming deeply conscious that courts of law so often lead to injustice as to almost warrant the questionable extremes of the "referendum" and the "recall." Thus, also, it is becoming painfully evident that while statesmen and publicists were never so well equipped for their work as at present, they are still pushing political and oratorical methods absurdly far in trying to settle by their aid such complicated questions, for example, as those of tariffs and the diminishing purchasing capacity of the world's monetary standards. Although there never was a time when men of merit received more ready recognition, there are yet those who would seek to divide the earnings and the savings of the industrious and the thrifty with the shiftless and the improvident. And while there never was a time when the rights and the opportunities accorded to women were so numerous and so universal, there are yet members of their sex who, reckless alike of property and life, would destroy laws which society has slowly and laboriously built up through ages of tentative effort and experiment.

This stupendous plexus of conflicting issues, this world-wide phantasm, one might say, of realizable and unrealizable ideas and ideals, may well be a source of despair to the enthusiastic philanthropist, to the hopeful humanist and to the pious religionist; for unless they take into account the secular extent of the time element involved and hence the painful slowness of the processes of evolution, they will not only fail to understand the issues in question, but will fail also to anticipate and to appreciate the improvements to which these issues will lead when fully wrought out. No one unacquainted with the essentials of the Darwinian theory and no one not animated by a patient and painstaking spirit of research can expect to gain anything better than a superficial view of the activities, aims and aspirations of contemporary life. The problems it presents are as much problems in biology, in anthropology and in all of the older branches of physical science, as they are problems in political economy and jurisprudence; although, strange as it may seem, we have hitherto held them to be, and may still expect them to be long commonly considered, problems belonging solely to the provinces of politics and religion. Out of this mixture of wisdom and unwisdom, out of this conflict of opinions of the masses and the

classes, and out of the hopes and the fears of a small minority of contemplative and constructive minds will come the advances to which a healthy optimism bids us look forward. But these advances may be rationally expected to come only slowly and falteringly, with many setbacks, and with direct benefits chiefly for our successors rather than for us; for biological science has taught us that the social organism works in general with extreme deliberation and works for the individual only as a relatively insignificant unit in his race.

And in keeping with this broader view of our relations to the larger part of the universe, the event we celebrate is especially noteworthy not so much by reason of its individuality as by reason of the type it represents and the trend of current thought it helps to express. For important as this center of research undoubtedly has been, and should long continue to be, to biological science in America, it is only one of numerous agencies for research now undergoing rejuvenation or now springing up in various parts of the world. The spirit of stolid conservatism and the spirit of reckless enquiry, alike inimical to the public welfare and to the progress of science, are being replaced in larger and larger degree by a spirit of patient investigation which seeks to substitute constructive for destructive work and to discover how the best interests of mankind may be secured under the inexorable restrictions imposed by that vastly larger part of the universe which we have hitherto so commonly and so blindly ignored. In this conscious effort to discover our relations to the environment in which we find ourselves and in this conscious recognition of the limitations of human existence and endeavor are to be found the most encouraging evidences of progress. We should guard carefully, however, against our instinctive tendencies to accept the widely popular fallacy that what we call research is either novel or of recent origin. Research, as we now understand the word, means simply a systematic application of the methods of science. These methods are as old, certainly, as written history, and they have undergone a tedious and painful development. We may no longer think rationally of the achievements of men as appearing suddenly any more than we may think rationally of other phenomena occurring in violation of the principle of continuity. All such vagaries, though still too common, belong to the Homeric childhood of our race. Neither are these methods the exclusive property or tools of any class or classes of men. What is new with regard to them is an increasing assurance in their validity as a means of truthfully interpreting and hence controlling within determinable limits the conditions of existence on our planet.

But in addition to these generally favorable circumstances for the appreciation and for the promotion of research, circumstances far more propitious, probably, than at any earlier epoch in history, there are many collateral considerations which obviously demand our attention if we are to make good use of the enlarged opportunities now becoming available to us in increasing measure. Along with the extraordinary advances of science and its beneficent applications in the nineteenth century there has come also an equally extraordinary development of private and public confidence in those advances and hence a desire for liberal endowment of research by individuals and by governments. This has been the case in our country especially. Captains of industry, philanthropists and legislators have manifested a spirit of altruism and a degree of foresight quite without parallel in previous experience. An unprecedented amount of funds has recently become available for research, and

this amount appears destined to increase as time goes on. We are thus confronted, in America, at any rate, by a relatively new set of problems for men of science, problems in finance, in administration and in adjustment of mutually helpful relations between novel research establishments and organizations already extant in the fields of education and other forms of altruistic effort. It is to some of the requirements which these problems demand of us as specialists in the domain of science that this address is more particularly devoted. What are the needs of the times, what are our personal duties and responsibilities as workers in science, and how should we seek to forward the improvements essential to further the progress of our race? To these and to allied questions your attention is henceforth invited.

It would appear quite unnecessary before an audience of this kind to further define the meaning of the word research. But it may be instructive to consider for a moment how far the popular mind, and how far many disciplined minds, may depart from the meaning we attach to the term. We should never forget that the investigator lives usually in the presence of majorities which do not understand him and that progress is largely conditioned by these majorities. Thus, to journalists and to their readers it would seem that research is akin to necromancy and that its results are produced chiefly by witches of the male sex, otherwise designated in the polite literature of our day as wizards. Closely akin to this infantile fallacy is the more subtile error entertained by a majority, perhaps, of our highly educated contemporaries, that the more remarkable results of research are produced not by the better balanced minds, but by aberrant types of mind popularly designated by that word of ghostly, if not ghastly, implications, namely, "genius." Out of these misconceptions, which require only the briefest examination for their rejection, arise volumes of fruitless correspondence and many directly serious obstacles to progress. They are evidently part of the intellectual rubbish we have inherited from the remote past; but unfortunately their obvious origin does not prevent well disposed inquirers from raising the questions whether research establishments will undertake investigations which are not scientific and whether they should not give special attention to eccentric rather than to normal minds. A clarification of ideas which will lead to a dissipation of these vagaries is one of the greatest needs of the day.

A similar clarification of ideas in the popular mind is essential to appreciate the distinction between the usual aims of the investigator and the usual aims of the inventor. Investigation and invention are so closely allied that they are often confounded with one another. Indeed, the investigator is often compelled to devise inventions to promote his researches and the inventor is often compelled to make investigations in order to perfect his inventions; while the secretiveness of the inventor has its correlative in the desire of the investigator to secure priority of publication, as in the naming of new species. But in general the objects of the investigator are mainly altruistic while those of the inventor are mainly egoistic; the one seeks to give freely to the world the results of his researches, the other seeks personal benefits by aid of letters-patent. It is plain, therefore, that while there is now room for contemporaneous and probably advantageous public altruistic and private egoistic organizations for the promotion of research, relations of complete reciprocity can not obtain between them. Thus, for example, the U. S. Bureau of Standards is giving the results

of its admirable researches to a multitude of highly productive and praiseworthy commercial organizations; but whether the inventions so promoted and protected by patent laws will result on the whole advantageously to society or to inventors is a question which remains to be determined. It is well known that as a rule inventors are a sadly disappointed class; and when we consider the great waste of effort and resources entailed by patent litigation, it appears plain that the aggregate of rewards arising from the egoism of the inventor is much less than the aggregate of rewards arising from the altruism of the investigator. We may look forward, perhaps, to an epoch of more advanced social development when the functions of patent offices will be abolished except as they may serve to register important improvements and discoveries. In the meantime, altruistic research agencies may not be properly expected to perfect inventions, to secure letters-patent for them, to defend inventors in suits at law or to exploit successful inventions.[2]

Another popular illusion which everywhere retards the evolution of research institutions is more specious and hence more dangerous than those just referred to. It involves all of the fallacies which have thus far rendered the highly developed mathematical theory of probabilities of limited application in the ordinary affairs of life. It manifests itself in various forms, but most commonly finds expression in the notion that research establishments should solicit suggestions, or busy themselves in casting drag-nets in the wide world of thought, or in dredging, as biologists would say, with the expectation that out of the vast slimy miscellanies thus collected there will be found by the aid of a corps of patient examiners some precious sediments of truth. By this method it is assumed that the entire range of possibilities for discovery will be included; and it is likewise assumed that no idea of value can escape the superhuman intelligence attributed to the examiners. There is thus available at last, the argument runs, a comprehensive way to utilize for any given case even the small soul of truth contained in that ancient and cautiously wise aphorism, "there may be something in it"; and the doors are thus opened also to the hosts of amateurs, dilettanti and paradoxers who stand ready to waste the time and the resources of research establishments in the pursuit of the obvious, the futile and the demonstrably unattainable.[3] Two simple facts will suffice to dispel this illusion. The first of these is that important advances in knowledge are far more likely to issue from the expert than from the inexpert in research. Indeed, the probability of extending knowledge by organizations conducted by disciplined investigators is so much greater than the probability of extending knowledge by the drag-net method that we not only may but should ignore the latter in comparison with the former. The second fact is that no competent examiner is willing to spend his energies in raking over the

[2] There is little doubt that an endowed organization, and possibly a state organization, for the promotion of inventions, could now be established with great advantage alike to society and to inventors. There are plenty of able inventors who would be glad to work on salaries and to give as freely to society the results of their labors as investigators do.

[3] Along with the amateurs and the dilettanti, who are not without certain commendable characteristics, there are to be counted also in great numbers cranks, quacks, charlatans, aliens and mountebanks. The paradoxers include especially arc-trisectors, circle-squarers and perpetual-motion men and women. It is amazing how easy it is for an individual of any of these classes to get letters of introduction to research establishments from our otherwise highly esteemed contemporaries.

contents of the drag-net. No more fruitless or thankless task could be assigned to an expert investigator than that of detecting the relatively microscopic quantity of truth to be found in the vast volume of error with which undisciplined minds would eagerly occupy his attention.

But the need of clear ideas on the subject of research is not limited to the majorities of our fellow men who are naturally preoccupied with other affairs. There are vexatious variations of opinion as to what research is, and great diversities of view as to how it may be effectively carried on, held even by its devotees and more especially by its nearest allies in the fields of education. Thus, adventure, exploration, the collection and naming of specimens and the tabulation of bibliographies, any or all of which may be incident to, are not infrequently mistaken for research by those engaged therein or seeking to contribute thereto. Similarly, it is often assumed that research is a harmless and a fruitless diversion in the business of education, and that it requires but a portion of the leisure time of those chiefly occupied with duties of instruction and administration in colleges and universities. On the other hand, some eminent minds maintain that serious and fruitful research can be advantageously pursued only in connection with work of instruction, while a few enthusiasts go so far as to suggest that the mental and the bodily vigor of an investigator can be conserved only in the stimulating presence of immature minds, otherwise known as students or candidates for higher academic degrees. Such eminent minds and enthusiasts entertain grave doubts as to the propriety of the existence independently of colleges and universities of research establishments. It is darkly hinted, indeed, that the latter may work harm, if not ruin, to the former by enticing the effective teacher away from his students and by checking the diffusion in order to promote the advancement of knowledge. Thus it has happened that sinister predictions and panicky sentiments have attended the development of a few research establishments in our own country and abroad during the past decade. We seem to have undergone a sort of intellectual flutter similar in many respects to that more profound emotional disturbance which followed the publication of the ideas of Darwin, Wallace and Spencer, a half century ago, and presaged the extraordinary development of biological science as we know it to-day.

Happily, the untoward features of this more recent agitation, features leading to numerous unrealizable ideals and to numerous necessary disappointments, are now subsiding; and the sense of humor and the sense of proportion so essential to the dissipation of mental aberrations are now regaining the ascendancy. Indeed, after a decade of wild conjecture and extravagant expectation on the part of many men of science and many educators, we may now venture, perhaps, to look squarely at the facts which confront us and to apply the rules of elementary arithmetic with some hope of adequately visualizing the relations which should exist between educational establishments, on the one hand, and research organization, on the other.

Confining attention to our own country, some of the salient facts and figures we need to contrast and to contemplate are the following:

The number of higher, or degree-giving, establishments in the United States is now upwards of six hundred; the aggregate annual income of these is upwards of one hundred millions of dollars; and the number of officials connected with them is upwards of thirty thousand.

On the other hand, the number of independent research organizations in the United States is less than half a dozen; their aggregate annual income is less than two million dollars; and the number of officials primarily connected with them is less than five hundred.

The overwhelming disparity between these figures should assure us that there is no immediate and no prospective danger of the usurpation on the part of the more recent research organizations of the rights, privileges and immunities so long, and in the main so justly, enjoyed by educational establishments. But these arithmetical data go further and serve to dispel other illusions which have much hindered the progress of research during the past decade. They show at a glance why the combined incomes of a few research institutions could not meet the deficiencies even of their nearest allies, to say nothing of meeting the limitless wants of numerous other establishments which have expected likewise to be supplied from the same source. It is probable, of course, that of the large aggregate income of our higher educational institutions only a small fraction is available for the promotion of research. This must certainly be the case if the vast amount of expert testimony available is to be taken at its face value. But here again there appear some obscurities that need clearing up. For, as already indicated, it is claimed by many of our highly esteemed academic colleagues that colleges and universities are not only specially qualified and equipped for the conduct of investigation, but that they are the real ancestral homes of this high calling. They seem to possess all of the desiderata except funds. They are like the farmer who has an abundance of fertile land, but who remains inactive because he lacks capital for the production of crops. And just as we would esteem it permissible to challenge any claim on the part of our farmer that he is an expert in husbandry and that his farm is a natural agricultural experiment station, so must we regard it permissible, if not highly desirable in the interests of progress, to question the claims of our academic colleagues. The simple truth seems to be that research has been and is still rarely regarded by the great majority of academic men and women as anything but an unimportant incident to the principal business of academic life. This principal business is the transmission from generation to generation of acquired learning; and it has been adhered to so generally and so rigorously in the past that until our own time educational institutions might be said, with only slight qualifications, to have been depositories of stationary thought. Moreover, in these days of decreasing pretensions and increasing fulfillments it is incumbent especially on those claiming superior qualifications and facilities for research to bestir themselves in order that they may secure that degree of independence which is indispensable to the effective pursuit of fruitful investigations. It is futile as well as incoherent to argue that the funds of the newer organizations could be better applied by the older ones, since we have not heard of the latter proposing to divide their incomes with the financially embarrassed, but often highly commendable, smaller colleges of the country. But in addition to this patent inconsistency on the part of the protestants there is an obvious and insuperable arithmetical obstacle in the way of an acceptable division of the incomes of a few research organizations amongst a multitude of educational establishments, however worthy and however selected.

All this leads up to a frank submission of the proposition that whatever may prove to be the working relations between research

organizations and educational institutions, they must be relations of reciprocity. This is a proposition which should be of special interest to your organization, since sooner or later you will be compelled to consider it. Trustees of such organizations, it is safe to predict, will not be disposed to surrender their rights or to delegate their duties. In this respect they will doubtless be found to be just like trustees of educational institutions. Biologically the two groups belong to the same genus if not to the same species, and under like circumstances the reactions of either group will be the same as those of the other. And is it not plain that such relations of reciprocity are the only permanently satisfactory relations attainable? The widely spread, if not prevalent, assumption that research establishments are mere disbursing agencies, waiting for suggestions of appropriate ways in which to apply funds, is creditable neither to those who entertain it nor to establishments which accept it. This assumption entails too readily the futilities of amateurism, the dangers of favoritism and all of the inefficiencies due to division of responsibilities and to scattering of resources. Thus, while it is quite true that a majority of the fundamental researches of the past have been accomplished by individuals and that they will continue to be so accomplished in the future, it should nevertheless be the primary purpose of a research institution to institute and to conduct research; to take up especially those larger problems not likely to be solved under other auspices, problems requiring a degree of organized effort and a continuity of purpose surpassing in general the scope and the span of life of any individual investigator. Such institutions, like colleges and universities, should expect to continue their work forever, or, at any rate, so long as they are able to add to the sum of that sort of knowledge which is verifiable and hence permanently useful to mankind.

But "how," it is often asked, and doubtless some of our colleagues here are now raising the query, "are the requirements of the worthy individual investigators in colleges and universities to be supplied?" To understand and to answer this question rationally we need first to learn how to distinguish endowments from incomes and then to appeal to our knowledge of mental arithmetic. An application of this much-neglected branch of an ancient science will quickly show that the income of no single research institution now extant, or likely to be founded, can come anywhere near meeting the wants of the great army of competent investigators now pressing for financial assistance to forward their researches. Indeed, neither in a single institution nor in all of those now existing combined, nor in a score more of such, will there be found sufficient funds to supply the world-wide and rapidly growing demand for them. When we are ready to appreciate these salient numerical facts we shall be able to make the next step essential to relieve at once the straitened conditions under which hosts of worthy investigators are now chafing and to respond more quickly to the urgent demands of society for obviously attainable and desirable improvements dependent on research. This next step should consist, first, in an appeal not solely to a few of the captains of industry and the philanthropists whose wisdom and benevolence have been so conspicuously manifest in our day, but to the entire class of such, whose aggregate number, as long since proved by the experience of charitable, educational and religious organizations, is legion. If a small fraction of the vast aggregate annual expenditures of such organizations were devoted to research

under competent guidance, it would go far towards an understanding and hence an amelioration of the adverse social conditions which have so long roused the sympathies but baffled the judgments of the majorities of our fellow men. And in respect to this appeal it is a most encouraging fact that there are in waiting, so to speak, everywhere in our country, at least, increasing numbers of intelligent men and women ready to endow research as soon as they can find trustees of research funds in whom confidence may be safely reposed.

Secondly, this step in line towards relief should consist in the development of larger opportunities for research and in the collection of corresponding endowments therefor, by universities. They must lose their leadership in research if they are obliged in any considerable degree to depend on other organizations for financial support. They should recognize that the ends of research are not limited to the highly worthy object of fitting candidates for the doctorate degree; and they should recognize that there is the amplest room for the simultaneous existence of educational institutions along with other organizations whose primary purpose is not the diffusion but the enlargement of learning. And in the adjustments now forming between these two classes of establishments there should arise the freest relations of reciprocity, especially as regards individual investigators. Much baseless fear has been expressed lest a few research organizations should rob academic staffs of their ablest men, as if those relations at first slightly unilateral might become increasingly or wholly so. It is of supreme importance to both classes of establishments, and particularly to progress in the immediate future, that eminent men should be free to pass from one to another of these establishments without encountering any administrative or other purely institutional obstacles. In fact, it should be esteemed one of the highest attainable objects of any institution to assist in the production of investigators whom other institutions are glad to offer desirable or superior opportunities.

And thirdly, relief should come in large measure through increasing appropriations of public funds to forward all of those numerous researches essential to the public welfare. These fall mostly in the fields of applied science and are often erroneously assumed to produce only the so-called "practical results" directly aimed at. But every investigator knows that the by-products of such researches are usually quite as important as, and often more important than, their anticipated products. A vast aggregate of such work is now carried on by the United States government, by states and by municipalities; and it should be observed that on the whole this work is well done, in spite of the contemptuous references one sees and hears occasionally to the conduct of scientific work under governmental auspices. In a republic destructive criticism of this sort has little weight, since it carries with it the illogical conclusion that our governors are, as a class, inferior to the citizens who elect them. What we much need in this, as in many allied governmental affairs, is less of destructive criticism founded on the shifting sands of partisan sentiments and more of constructive criticism founded on adequate knowledge of biology and anthropology. As a matter of fact and of justice it must be admitted that the aggregate of high-class work of research accomplished by the bureaus of the United States government in recent decades compares very favorably with the corresponding aggregate accomplished by educational and other establishments of our country during the same period. We who labor in the latter establishments, therefore, have no ade-

quate reason to suppose that our reputations may be much improved by invidious reflections on the methods in science followed by men who happen to live "in Washington." Here again it is useful to remember that we and they belong to the same species.

But in order that any measures of relief and of response to the pressing demands of society may become adequately and progressively effective, certain other requirements of greater importance must be realized. These requirements must be supplied chiefly by men of science. To a far greater extent than ever before the methods and the applications of science are concerned with the daily affairs of domestic, national and international life. Ours is an era of unequalled opportunities in science; but it remains in part, at least, to be demonstrated whether the types of men called scientists, whom it has taken many generations to evolve, are prepared to meet the responsibilities as well as the duties now falling upon them. It is an open secret that as a class doctors in science are on trial, and properly so, in so far as they may suggest remedies for the body politic. In the evolution of society they are a sort of "fourth estate" and the latest in the order of human development. It is not so long ago, quite within the recollection of some here present, when society was guided almost wholly by three other classes typified by the man in the saddle, by the man in the pulpit, and by the man on the bench. The rôle of the man of science as manifested somewhat sensationally to the popular mind, for example, in the conduct of industries, in the control of epidemics and in the construction of the Panama Canal, has been recognized only recently as one of vital importance to communities and to states. It is especially incumbent on us, therefore, at this juncture, to put our scientific houses in order and to be ready to demonstrate the validity of whatever claims we may set up by the production of work which will stand on a basis of verifiable merit. It would be unscientific, and inimical to progress, to ask for easier conditions of entrance into the world's affairs, from which as a class we may no longer advantageously either hold ourselves aloof or be debarred by other classes.

Fortunately, there is now little danger that the prejudice and the ignorance which provoked so many wordy wars and so long contested the advent of the "fourth estate," will exert anything like such sinister influences in the future as they have exerted in the past. Science is ever ready and willing to settle matters in dispute by the arbitrament of demonstration, and the conclusiveness as well as the fairness of this procedure is now nearly universally conceded. Indeed, the effectiveness of the methods of science is now not only generally recognized within each of the older "estates" just referred to, but even the more conservative members therein are making and projecting scientific researches with a degree of enthusiasm which compels our admiration. The dangers which beset us are rather dangers of popular over-confidence in our methods, of amateurism and dilettantism and of premature generalizations. The prevailing optimism needs to be chastened by the reflection that the millennium is not in sight, that sound research means arduous enterprise and that advances in knowledge come, as a rule, only after prolonged and even painful effort. In the meantime, while guarding carefully against these dangers, it is the part of wisdom to take every legitimate advantage of the present highly favorable attitude of our contemporaries towards research. We of the "fourth estate" need especially to fraternize with our colleagues

of the other three "estates" and likewise with the still larger and equally favorably disposed groups of our contemporaries in the world of trade, commerce and industry. It is well for us to study them lest we should misunderstand them. We need their aid now more than they need ours; and it should be borne in mind that when any proposition is to be voted upon they are overwhelmingly more numerous.

Time does not permit more than passing reference to the important but as yet little studied subject of research publications, their proper distribution and their adequate popularization; nor to the more important, though less debatable, subject of administration, including what are too often contemptuously regarded by men of science as unattractive if not unessential details of fiscal business; nor to the still more important and complex but little understood subject of boards of trustees, the best methods of choosing them and their proper relations to research organizations.[4] It must suffice here to call attention to them, among many other subjects, as specially in need of patient investigation by men of science. They are subjects, however, whose elucidation may be deferred. An adequate understanding of them will come, apparently, only after the more elementary considerations already dwelt upon are visualized and appreciated. Passing these considerations rapidly in review, the salient needs of research and some of their numerous corollaries may be advantageously summarized in paraphrase and in aphorism even at the risk of apparent dogmatism:

We need first to recognize that in its inclusive aspects research is in scope coextensive with the universe of which we form an insignificant part, but in which we are obliged to play the significant rôle of interpreters if we would make the best of our opportunities. The experience of our race has demonstrated that by study and hence by understanding of this universe the roads to progress may be found. The methods of research are the methods of science. They are not of recent origin. They have undergone an evolution extending far backwards towards the era of primitive man. What is new about them is a widely general and rapidly increasing recognition of them as the most trustworthy methods man has devised for the discovery of truth and for the eradication of error. Along with this recognition there has gone on, and is still going on, a gradual elimination of Homeric illusions and fallacies; so that male as well as female witches must be abandoned by all except the more atavistic, while the appellation "genius" in the singular as well as in the plural is becoming one of doubtful compliment. We are coming to understand also that while there may occur flashes of wit, and even of wisdom, from abnormal types of mind, the more effective emanations of both wit and wisdom are to be expected from normal and patiently contemplative types. And thus the more striking results of research, quite commonly in the past attributed to wizards and to genii, and still so attributed by a majority, probably, of contemporary writers for the popular press, are now understood by the thoughtful to be products rather of industry, sanity and prolonged labor than of any superhuman faculties.

Out of this rational appreciation of the methods of science have arisen quite natu-

[4] An important contribution to this subject has been made by President Eliot in his volume on University Administration (Houghton Mifflin Company, 1908). All such works, however, are generally held to be "too theoretical" by the average man who prides himself on being "practical." In his assumed freedom from theory he often adopts the obviously erroneous theory that there is no room for progress or improvement in the conduct of such affairs.

rally, but relatively suddenly, unprecedented demands for research, on the one hand from communities and states, and on the other hand from academies, societies, institutes and universities. We should observe, however, that this intellectual uprising dates back at least a half century, to about the time when your science was emerging from the limbo of "natural history" in which it had been left to slumber by Pliny the elder. It is part of the general uprising of the nineteenth century of which the multiplication and fruitful activity of scientific societies in America is another surprising and gratifying manifestation. Quite naturally, also, along with this greatly enlarged appreciation of the value and desirability of research there has come a corresponding demand for enlarged facilities and particularly for funds. This demand, like most unanticipated demands, is in the aggregate vastly greater than the present or possible supply, but not greater than can be met if pruned of its adventitious appendages. Research and research organizations are somewhat in danger of being swamped by an excess of symbiosis.

In these circumstances there is constant need of the caution and the deliberation which distinguish scientific investigation from impulsive and emotional mental conduct. We should frequently recall that the characteristic defect even of deliberative bodies is lack of deliberation. We need constantly to apply our well-known methods of research to the questions confronting us. Instead of following precedent, we should in general avoid it. When, for example, a research fund is established we should not make haste in academic fashion to set up poor-boy scholarships and roving fellowships to be awarded to the amateur and to the tyro, but we should seek to originate and to conduct research under the auspices of competent and responsible investigators. And as regards research in academic circles, we need to fix attention rather on the professors who are qualified to extend the boundaries of knowledge than on their pupils. These latter, if worthy of the name, will require little formal instruction in the presence of evolving discoveries and advances; moreover, they must learn early to think with their own heads if they may hope to become either competent teachers or leaders in work of research.

And finally, men of science, if they are to meet the requirements now demanded of them, need more of contact with, experience in and sympathy for, ordinary business life. We are as a class of too recent monastic descent to fit comfortably in our present social environment. The man of affairs does not understand us, and hence often looks upon us with suspicion or even with contempt. He is generally sure that the man of science can know little of finance and of other affairs vaguely emphasized by the adjective "practical." Argument concerning this matter is idle in the face of existing conditions which determine majorities in boards of trustees and in legislative assemblies. Nor would it be the part of wisdom to change abruptly if we could the present course of evolution in affairs of administration. We need to accept the situation as we find it and to qualify for gradual entrance into, and participation in, the details of this ordinary life. It will not be taken for granted, for example, that we can keep accounts and live within income, but a positive demonstration will be accepted without protest. It may be easily shown to our satisfaction by *a priori* reasoning that men of science are no more likely to wreck corporations than financiers, general managers or promoters, but proof by numerous concrete examples must be forthcoming from us. And in proving capacity for trustworthiness in these, to us,

new fields we should avoid the manifest errors of our business predecessors. Agreeing with Dr. Johnson's astronomer that "the memory of mischief is no desirable fame," we should not seek, for example, to perform the academic feat of capitalizing deficits. Even if there were a body of alumni to which appeal might be made in distress, such a feat would be unworthy of a research organization. Above all, research organizations should embrace the great advantages that come from open audit and truthful publicity in all financial affairs. We should accept these and the other conditions and limitations of our environment to which attention has been called, not in a spirit of unreflective meekness, nor in a spirit of impatient defiance, but in a spirit of philosophic equanimity, confident that the scientific methods of observation, experiment, comparison, demonstration, generalization and verification will ultimately work out adjustments to the permanent advantage of our successors, if not to the ephemeral advantage of ourselves.

R. S. WOODWARD

ADDRESSES AT THE DEDICATION OF THE NEW BUILDINGS OF THE MARINE BIOLOGICAL LABORATORY[1]

THE subject of biology possesses immense significance for human thought and action. If the biology, the sociology, the philosophy and whole mode of thought of the twentieth century differ quite radically from those of the mid-nineteenth century, it is largely because the biological investigations of Lamarck, of Darwin and of many others founded the evolution theory, the future development of which is one of the main problems of biology.

The cell-theory, another great generalization of biology, revolutionized the study of pathology, the basis of medicine, besides furnishing the indispensable foundation for all future biological studies. The conception of the physicochemical constitution of protoplasm, or living matter, is a third great contribution of biological science of inestimable significance for science and philosophy.

Biology is related to the most practical affairs of life: to medicine, of which it forms the indispensable foundation, to hygiene and public health, to many problems of agriculture and animal industry, and to fisheries problems. Economic entomology, parasitology, protozoology, etc., are practical branches of our great subject; not to mention the fundamental principles of the mooted subject of eugenics. The advancement of biology is one of the most important considerations of modern society.

Even such an intentionally incomplete statement of the significance of biology may appear exaggerated. But nothing is more sure than that the acquisition of knowledge increases man's control of nature, and that the science of biology, although still in an early stage of its development, promises control of those uncertainties of practical human life which are most perplexing and dangerous to the race.

The significance of the present occasion is to be found only partly in such general considerations. This laboratory represents one of the forces that have to be reckoned with in this general situation. But it is to the special significance of this occasion that I would more particularly direct your attention.

The sea-shore is undoubtedly the ideal situation for a biological station, because marine life offers certain valuable opportunities for study that are unique. These are given in such a situation as ours, and we relinquish none of the opportunities of inland laboratories. Louis Agassiz, in America, and Anton Dohrn, in Europe, were among the first to organize seaside laboratories; about the same time, 1872, Agassiz founded his station on the

[1] In addition to these shorter addresses and the address of Dr. R. S. Woodward, printed above, an address was made by Professor Edwin G. Conklin, of Princeton University, who, on account of his absence from the country, was unable to prepare it for publication. Mr. C. R. Crane, president of the board of trustees and donor of the building, presided and presented the speakers.

neighboring island of Penikese, and Dohrn his station in Naples.

This laboratory is in a very real sense a lineal descendent of Agassiz's station. Our immediate predecessor was the Annisquam Laboratory organized to serve the same ends as the Penikese school, and the forces there were supplemented and transferred to Woods Hole in 1888.

It is not sufficient that a laboratory should merely be established and equipped. It must be properly organized and manned. In some respects our laboratory has an unusual form of organization. As Mr. Crane has well said, freedom is its dominant character; the freedom of a democracy of learning. Our corporation, numbering 300, extends into a large proportion of the institutions of learning of the country. Our board of trustees, chosen by the corporation, includes representatives of various branches of the biological sciences in many of our leading institutions. The laboratory is owned and controlled by the people whom it serves; and this is the essence of a democratic organization, the only assurance of freedom of development.

The laboratory thus organized stands for the advancement of the biological sciences by research and by teaching. We have not believed it wise to divorce these two functions of learning. The research creates an atmosphere in which teaching is most vital, and the teaching humanizes the research by bringing it constantly in contact with the needs of students, besides serving the essential function of training future investigators.

Freedom of organization is our one watchword. Cooperation is our other. Both are vital, and they are interdependent. When people are free those of similar interests naturally cooperate, so long as they respect freedom. And so we have a union of forces of scientific men, and through them of institutions that they represent in order to create conditions as ideal as possible for the progress of science.

The new building stands for a certain stage reached in the evolution of this democratic institution; it stands for recognition of a certain degree of demonstrated stability; and for a certain amount of assurance of permanence. And so we rejoice in the present occasion, and have asked many of our friends to join with us in dedicating this building to the ideals of research, of teaching and of cooperation in freedom of spirit.

This magnificent building which we dedicate to-day is the most efficient instrument of research in the hands of biologists. For its beauty and enduring strength we are indebted to the great architect, Charles Coolidge, who rendered his services freely; and for its convenience, adaptability and sufficiency to Dr. Drew, with whom the perfection of every detail has been a labor of love.

We must not forget on this occasion to honor the memory of our greatest leader, Professor Whitman. I would that he had lived to see this day; and, as he valued the things of the spirit so infinitely above the material, I hope that he would find that the spirit of the present stage of our institution matches its material equipment.

R. S. Lillie

I appreciate the courtesy that has been extended to me by the invitation to attend these exercises. I have gladly accepted that invitation on behalf of the bureau I represent, because I feel it to be a pleasure that may properly be enjoyed and a duty that should not be neglected, to testify by my presence and words to the interest which the Bureau of Fisheries has in the opening of this new building and in the larger field of usefulness which is hereby presented to the Marine Biological Laboratory.

From Secretary Redfield I bring a cordial message carrying hearty congratulations, appreciation of the spirit which has actuated the donation of this magnificent edifice, sympathy with the past and future work of this institution, and the assurance of his desire to have the scientific activities of his department, here and elsewhere, in genuine cooperation with and in aid of biological research.

My dominant thoughts on this occasion are of those who once labored here but are no longer with us. I have been thinking of the satisfaction with which they would have en-

tered into this day's exercises. I need not name all of them, but I recall, as you will, Peck, Ryder, Montgomery, Gardiner, and especially Whitman; and one other, the pioneer who really discovered the biological advantages of Woods Hole as early as 1869, and did as much as any one else to inaugurate the movement which has made this the most noteworthy American center for marine biological research. I refer, of course, to Spencer F. Baird.

I have been asked to speak of the cooperation that should exist between the Bureau of Fisheries and the biologists and their institutions; but that is too large a subject to handle adequately in the few minutes that have been allotted to me.

It is perhaps quite unnecessary for me to state that the Bureau of Fisheries is always ready to lend to biologists substantial aid and effective cooperation compatible with its functions and with the purposes for which it receives support from congress. The various phases of this cooperation need not be mentioned, but there may be cited, as an example, the scientific expeditions to which the *Albatross* was assigned, under Agassiz, Jordan and others, which have resulted in larger additions to knowledge of the life of the sea than have come from any other source, not even excepting the *Challenger*.

On the other hand, many of the leading biologists of the country have rendered noteworthy service to the bureau in investigating fishery and cognate subjects. In the capacity of investigators for the bureau or as the recipients of the courtesies at its laboratories, on its vessels, or in the field, a very large proportion of the prominent American biologists of the last quarter of a century have cooperated in the furtherance of science. At the present moment we are favored by cooperative relations with the representatives of the biological departments of 10 state universities and of as many other front-rank universities, to say nothing of various other institutions of learning.

I will take this opportunity to call attention to the fact that, in addition to the two marine fisheries laboratories now maintained by the Bureau at Woods Hole and Beaufort, it is expected that during the next year work will be commenced on a third marine biological station, to be located at or near Key West, where the wonderful fauna of the Gulf Stream and of the abysses over which it flows, and of the coral reefs and the shoal waters back of them, will furnish unrivaled opportunities for research. Furthermore, if a bill now before congress should become a law, a fourth station will be established on a site which will render accessible for study under government auspices one of the rich biological regions of the Pacific coast.

During the present summer there has been opened a fresh-water biological station, located on the Mississippi River at Fairport, Iowa. It has a large laboratory building, an abundant supply of crude and filtered river water, an extensive pond system and a general equipment that should render it an important factor in the study of the biology of the waters of the Mississippi Valley.

All of these laboratories are, or will be, freely open to qualified men of science, under such restrictions only as are required by good administration.

Here at Woods Hole, the friendly relations that already exist should be extended. The two laboratories have different functions and occupy different fields. There is no reason why any feeling of rivalry should exist. There is every reason why mutually helpful and close cooperation should prevail. Mention may be made of some of the ways in which the two institutions may profitably work together:

(*a*) Exchange of material where research is being conducted on a given subject at one laboratory and not at the other. For instance, the Bureau of Fisheries is now conducting at Beaufort, and will conduct next summer at Woods Hole, a comprehensive study of the post-embryonic development of economic fishes, a very important subject to which practically no attention has heretofore been given. Suitable material obtained by the Marine Biological Laboratory in its towings and otherwise would be valuable and most acceptable.

(*b*) Exchange of information between the directors concerning the subjects under investigation at the respective laboratories, with the view to prevent duplication of work, but particularly to advantageously supplement at one laboratory work which in some of its phases may be under way at the other. For instance, certain work at the Marine Biological Laboratory may have economic connections which would not be given much consideration. Probably an investigator at the Fisheries Laboratory could be assigned to this side of the subject to the mutual advantage of both workers, economy of material and effectiveness of effort. Conversely, while the Fisheries Laboratory is concerned with investigations more directly related to the fishing industry, there frequently arise in connection with them collateral, more abstract, problems which would perhaps appeal to investigators at the Marine Biological Laboratory.

(*c*) Reciprocal access to daily collections. It frequently occurs that when no one at a laboratory has an interest in a certain organism, or classes of organisms, the material collected is either thrown away or imperfectly cared for. If when the collections are brought in a competent person from the other laboratory, and familiar with its needs, could be given an opportunity to examine the collections, or at least the rejected material, much now wasted might be utilized.

(*d*) The effectiveness of the collecting could probably be increased by such cooperation as would prevent duplication in the fields covered. This could be arranged by an understanding of mutual requirements and the cooperation of the collectors.

I share the feeling entertained by many others that a new era in American biological science is now dawning; and that, under the inspiration and stimulus afforded by Mr. Crane's noble gift, the day is not far distant when Woods Hole will come to be generally recognized abroad as well as at home as the world's biological Mecca.

Hugh M. Smith

Time Ratios in the Evolution of Mammalian Phyla. A Contribution to the Problem of the Age of the Earth

Considered as a historic science, geology has not yet solved its first problem. There is as yet no satisfactory way of estimating the age of the earth and the length of geologic periods. The various methods that have been devised to compute it are all subject to such large factors of uncertainty dependent upon questionable assumptions, that the most that can be claimed for them is that they indicate the order of figures which should be assigned as the antiquity of geologic periods. The relative length of the periods one with another can usually be more definitely gauged. But the translation into years is a matter of wide divergence of opinion and no real proof that any of the results are even approximately correct.

It is quite true that various estimates have been made by geologists and physicists resulting in figures which are of the same order of magnitude and in reasonably close agreement, although derived from independent sources. This might be taken as evidence that the age probably lies within these limits. But in fact it does not prove any such thing, for it rests in every case upon the assumption that the activities, whose accumulated results are the measure of the length of time that they have been in action, have proceeded in past times at the same pace as at present. This is not only unproved, there are strong reasons for believing it widely different from the fact.

There is no occasion to review these methods of computation or to point out other unprovable assumptions. Every competent discussion of the subject has sufficiently called attention to them.

What I have to contribute is the suggestion of a possible measure derived not from inorganic, but from organic evolution. It is approximate indeed, and relative, based like the others upon assumptions which can not be proven. But it is perhaps—I dare not say more—free or partially free from subjection to the varying intensity of inorganic activities

which vitiates in common all calculations based upon the assumption of their constancy.

In working upon the numerous phyla of vertebrate animals, especially of mammals whose evolution is recorded in our Western Tertiaries, I have been impressed with the fact that they seem to have a fairly constant maximum rate of progressive evolution. The rate of alteration in structures that are being changed adaptively to some changing environment or habit is fairly uniform, comparing one phylum with another. Where concentrated upon one element of change or a few, it is more rapid; when distributed into a great number of alterations of a complex structure it is slow. Some structures are much slower to change than others—notably this is true of the teeth as compared with the bones of the skeleton.

It is essentially a constant progressive change. Where we find sudden jumps of any considerable magnitude the explanation is always at hand, and usually obvious when the circumstances are studied judicially, that we are dealing with an imperfect record, and the breaks are due to migration or to unrecorded lapse of time. To prove this point—a disputed one, I am well aware—would take me too far afield. I must rest on the assertion that twenty years study, in field and laboratory, of American fossil mammals, has brought me to the conclusion that the evolution of their phyla took place through the cumulation of minute increments of structural change, at a rate which, whether concentrated upon one feature or distributed over many, presents some approach to a uniform maximum.

I fully believe that the change is due to the pressure of the environment, acting through selection upon individual variations. Whether these be mendelian or fluctuative in their law of transmission is immaterial. The point is that they are minute, well within the limits of a species as conservative paleontologists draw those limits.

If they are accumulated through selective action of the environment, how can they be said to be in any sense free from the varying rate of change of inorganic activities which vitiate calculations based upon the constancy of their action. If the environment is changing rapidly at one time, slowly at another, will not this be reflected in the rate of change of any phylum of living beings? Undeniably this is true. Yet there does appear to be a maximum rate of change as above outlined, and environmental change exceeding that limit results in migration and extinction, not in structural alteration. Moreover, a large part of the structural evolution which we can observe must be in reaction to the pressure of the biotic, not the physical environment. A large portion of the progressive structural change is advantageous to the animal under any circumstances, whether or not the physical environment changes. This is peculiarly true of increase in brain capacity; it is partly true of increase in mechanical perfection of the structure leading to increased speed, better tooth mechanism as well as numerous changes not recorded in the skeleton.

It would seem therefore that there is a maximum rate at which alterations in the structure can take place. I suppose this rate to be conditioned by two factors, individual variability in the organism, and selective processes under the conditions obtaining in nature. At all events the fact stands as of record, proved and confirmed by innumerable instances, that the evolution of any direct phylum does take place through cumulation of minute changes, at a rate which, allowing for concentration upon one element of change or dispersal over many, does present a considerable degree of uniformity in corresponding parts, whether of the same phylum at different times or of different phyla at the same time. This rate may often not be attained, but I can find no convincing evidence that it can be exceeded.

The amount, variety and fundamental character of the differences thus accumulated are the practical measure of our systematic classification. A difference or group of differences of small amount, yet distinctly beyond the limits of individual variation, is customarily regarded as specific. Differences of a decidedly larger order are considered generic, and so on. It would perhaps be a fair average estimate

to say that one genus differs from the next ten times as widely or fundamentally as one species from its next neighbor. There is no sort of exact rule in the matter, but this would perhaps represent the average opinion to which each systematist endeavors to conform in arranging the group upon which he is working.

Now if the above conclusions are warranted we may find in the recorded evolution of various well-known phyla a rough measure of the relative length of the epochs covered by its evolution. In instance we may take the evolution of the horse. This phylum as represented in the American Tertiaries I believe to be a direct phylum so far as the genera are concerned; the relation of the species to the direct line of descent are mostly immaterial to the present discussion.

Equidæ (Direct Phylum)	Relative Amount of Structural Difference from Preceding Stage	Geologic Epochs
Equus caballus, etc.	1	Recent
Equus scotti, etc.	10	Pleistocene
Hipparion	10	Pliocene
Merychippus	15	Miocene
Parahippus	5	
Miohippus	5	Oligocene
Mesohippus	15	
Epihippus	10	
Orohippus	10	Eocene
Eohippus		Paleocene

It would be possible to verify these estimates of structural differences by comparative measurements. But it would be an enormous task. To select a few of the great number of structural differences for measurement would be almost certainly misleading; to average them all would entail many thousands of measurements for each species or genus compared. The final result might be twice as much or half as much as the estimate I have given; it would certainly not be ten times or one tenth as great. The margin of error for each estimate here given is not to any great extent cumulative for the whole series. The errors would therefore tend to balance to some extent, and the margin of error for the whole series would be less in proportion. For these reasons, and because of the doubt already expressed as to whether the maximum rate of evolution is really a constant, I have not thought it worth while to verify the estimates by measurements.

From the beginning of the Pleistocene to the present time, the evolutionary change in the phylum is measured by the difference between the modern species and the nearly allied species found in the Aftonian and other equivalent formations of early interglacial time. During the Pleistocene there has been a great deal of migration and shifting of faunas; the actual evolutionary change in this or any other mammalian phylum is notably small. It is perhaps one tenth the amount of structural change that separates *Equus* from *Hipparion* of the late Miocene and early Pliocene. *Hipparion* in turn differs about as much from *Merychippus* as it does from *Equus;* the estimated structural difference between the earlier stages is represented by the remaining figures in the column. Adding up these figures, we find that the amount of structural change in the *Equus phylum* during the Tertiary is 85 times the amount of Pleistocene evolution. So far as this is a measure of geologic time, it means that the Tertiary from Suessonian upward was 85 times as long a period as the Pleistocene. To this should be added a considerable figure for the Paleocene, whose length based on the evolution of other phyla might be assumed at 10 or 15 times the length of the Pleistocene. Briefly then, on this basis we should assume that the entire Tertiary is about 100 times as long as the Pleistocene, dating the latter from the first great glacial advance.

This is greatly in excess of the proportion usually assigned. But the Pleistocene was a time of extreme activity in sedimentation, denudation and other inorganic activities whose rate affords the basis of the various calculations that have been made. The amount of Pleistocene denudation, the thickness of its sediments, would hence give a greatly exaggerated measure of its length in time as compared with the whole of the Cenozoic.

The various other phyla of mammals support these proportions fairly closely. None are

quite so complete, direct and obvious in their structural change as the Equidæ. But the results obtained by a careful consideration of the phyla of Camelidæ, Rhinoceridæ, Tapiridæ, Canidæ, etc., do not appear to me to differ materially.

It is only in a very general and tentative way that we can apply these standards to the Mesozoic. A comparison of the amount of evolution in vertebrates between the end of the Permian and the end of the Cretaceous in comparison with the maximum change from the end of the Cretaceous to the present day, gives in turn the impression of a distinctly higher order and more fundamental quality of change. My impression would be that each of its four periods, Triassic, Jurassic, Comanchic, Cretacic witnessed structural changes in vertebrate phyla as extensive and profound as those that took place in the Mammalian phyla during the Tertiary. As to the Palæzoic, I have no basis for an opinion. It should be remembered that it is the maximum rate of change that is used as a measure. Many races, more often many characters in a race, changed slowly or not perceptibly.

It will be obvious that, if these proportions hold true, an estimate of the length of the Pleistocene will afford a measure of the length of the Tertiary and older periods in years. But the estimates of Pleistocene time differ enormously. The lowest estimate is perhaps by G. F. Wright, who will not allow more than 25,000 years. At the other extreme stand Penck and other authorities with estimates of 1,500,000 years or more. The more moderate figures of 50,000 to 200,000 years generally adopted seem more probable than either extreme. According to the proportions above estimated of Tertiary to Pleistocene time, we should have

Pleistocene	Tertiary	Mezozoic
25,500 years (Wright)	2½ million	10 million
100,000 years (Walcott)	10 million	40 million
1,500,000 years (Penck)	150 million	600 million

If the proportions usually assigned to the Paleozoic be correct, it was as long as or longer than Mesozoic and Tertiary combined. This would give twenty-five million years for the whole of the fossiliferous record upon the extreme figures of Professor Wright; on Walcott's estimate over 100 million, and on Penck's over 1,500 million years. For various reasons I am disposed to believe that the relative length of the Paleozoic should be revised upward, but the estimate of ten million years for the Tertiary and forty for the Mesozoic does not seem unreasonable.

W. D. MATTHEW

AMERICAN MUSEUM OF NATURAL HISTORY

SCIENTIFIC NOTES AND NEWS

AMONG the large numbers of American scientific men and university professors now detained on the continent and in England, probably the most serious inconvenience is suffered by the surgeons who attended the recent congress in London, some nine hundred of whom are said to be unable to obtain passage home. The only serious difficulty so far reported is the arrest and imprisonment of Mr. and Mrs. Archer M. Huntington in Nuremburg, Bavaria. Mr. Huntington is president of the American Geographical Society, and it is said was making a study of aeronautical routes.

PROFESSOR ELIE METCHNIKOFF, assistant director of the Institut Pasteur, will next year celebrate his seventieth birthday and the fiftieth anniversary of his doctorate. A committee has been formed, under the presidency of Dr. Roux, director of the Institut Pasteur, for the celebration of the anniversary which will include the publication of a "Festschrift."

MR. MARCONI has had the order of the Honorary Grand Cross of the Victorian Order conferred upon him.

AMONG those upon whom the University of Aberdeen conferred honorary degrees at the recent meeting of the British Medical Association were Mr. W. T. Hayward, Mr. T. J. Verrall, Sir Victor Horsley, Dr. Archibald Garrod and Sir John Bland-Sutton.

THE first presentation of the Saville medal, established by the West End Hospital of Nervous Diseases, London, in memory of the late

Dr. T. D. Saville, has been presented to Dr. Knowles Boney.

Mr. A. T. Bradlee has been awarded the medal offered by the National Association of Cotton Manufacturers for investigations upon the effects of moisture in testing cotton yarns and fabrics.

In addition to those already named in Science Dr. C. C. Abbot, director of the astrophysical observatory of the Smithsonian Institution, will attend the Australasian meeting of the British Association as the guest of the New Zealand government.

The foreign medical men who attended the Aberdeen meeting of the British Medical Asscciation included: Professor Stéphane Leduc of Nantes; Dr. E. Pontoppidan, professor of medical jurisprudence at Copenhagen; Dr. Clemens von Pirquet, professor of pediatrics at Vienna; Dr. Umberto Gabbi, professor of tropical medicine at the University of Rome; Dr. Karl Jung, professor of psychiatry at the University of Zürich; Dr. Alban Bergonié, professor of biological physics at the University of Bordeaux; Dr. H. Morestin, professor of surgery in the University of Paris; Dr. Rist, professor of clinical medicine in the same university; Dr. Fritz Frank, professor of midwifery at Cologne; Professor D. S. Demetriades, of Athens; Professor Adolf Onodi, the laryngologist of Budapest, and Dr. J. R. Macleod, professor of physiology, Western Reserve University.

Mr. Watson Nightingale, B.S. (Mass. Inst., '14), has been sent by the Bureau of Fisheries to make a series of microscopic observations of plant and animal life on the Grand Banks off the coast of Labrador.

Messrs. F. E. Matthes and F. C. Calkins, of the U. S. Geological Survey, have returned to the Yosemite region, in California, to resume the geological and geographical studies they began last year.

The American Museum of Natural History has sent two expeditions from the department of vertebrate paleontology, the first in charge of Mr. Barnum Brown, to the Red Deer River of Alberta, Canada, to collect Cretaceous dinosaurs, and the second, in charge of Mr. Albert Thomson to Agate, Nebraska, to secure additional *Moropus* skeletons.

Professor J. C. Bose, of Calcutta, will deliver a lecture before the Royal Society of Medicine, London, on October 30, on the modification of response in plants under the action of drugs.

The biennial Huxley lecture will be delivered by Sir Ronald Ross, K.C.B., F.R.S., at the Charing Cross Hospital Medical School on October 1.

We learn from *Nature* that it was decided at a meeting of alpinists held at Zermatt on July 25 to commemorate the fiftieth anniversary of the first ascent of the Matterhorn (falling on July 14 next) by the erection of a marble statue of Mr. Edward Whymper at the age he was when he first climbed the Matterhorn. The pedestal is to be of granite taken from the Matterhorn and the monument is to face the peak. The memorial will also commemorate Lord Francis Douglas, Mr. Hadow, the Rev. C. Hudson, and the guides, Michel Croz and the two Tangwalders. The cost will be borne by subscriptions. Mr. Justice Pickford, president of the Alpine Club, is to be invited to become the honorary president of the memorial committee. Dr. A. Seiler was appointed treasurer, and Mr. J. Grande, of Berne, honorary secretary.

The death is announced of the Rev. Dr. Stephen D. Peet, editor of the *American Antiquarian and Oriental Journal*, which he established in 1878 and conducted for thirty-two years.

Dr. R. J. Anderson, professor of natural history and geology at University College, Galway, has died at the age of sixty-five years.

The Russian government's ice breaking steamer *Taimyr* arrived at Nome, Alaska, on August 4, and left the following day for Wrangell Island to take off the twenty-one men who found refuge there after the wrecking of the Stefansson exploring ship *Karluk* in the ice north of Herald Island last January. The United States revenue cutter *Bear* sailed for Wrangell Island via Point Barrow on

July 21. After she sailed news was received that the ice about Wrangell Island was unusually firm, and the Russian government decided to send the powerful *Taimyr*, which can cut a way where the wooden *Bear* would be helpless.

ACCORDING to its program, the ninety-seventh annual meeting of the Swiss Scientific Association will be held at Berne, on August 31 to September 3. The general addresses include: "The Influence of Natural Science on Modern Medicine," by Professor H. Sahli, of Berne; "The Synthetic Dyes," by Professor Noelting, of Mühlhausen, and "The Primates of the New World," by Dr. H. Bluntschild, of Zürich. The association meets in nine sections for the reading of special papers.

WE learn from the *Journal* of the American Medical Association that an international school hygiene congress will be held in Brussels next year. The program takes up the following subjects: school buildings and equipment; medical inspection of urban and rural schools; prevention of contagious diseases in schools; the teaching of hygiene to teachers and parents; school hygiene in its relation to physical education of children; methods, syllabuses and school equipment in their relation to school hygiene; school hygiene in its relation to the children, and school hygiene in its relation to adolescents. The congress is under the patronage of the King of Belgium, and under the auspices of the National Institute of Pediology and the Belgian Pedotechnic Institute. The committee of organization is presided over by M. J. Corman, director general department of sciences and art, and Dr. J. Demoor, director of the Free Institute in Brussels.

ACCORDING to the *Bulletin* of the American Geographical Society a large relief model of the Yosemite Valley is being constructed at the Office of Public Roads in Washington for the government exhibit at the Panama-Pacific Exposition. It is twelve feet long, six feet wide and carries relief to a height of 18 inches. The vertical dimension is not exaggerated, and as a consequence all features are shown in their correct proportions. Indeed, so rugged is the topography of the Yosemite Valley, that any increase in the vertical scale would have resulted in a peculiar, distorted appearance of the great cliffs, domes and spires. The model is being executed, with painstaking exactness, by an expert model maker, and is based upon the detailed topographic map of the Yosemite Valley, prepared in 1905–06 by Mr. F. E. Matthes, of the United States Geological Survey. Portions of this map were enlarged photographically to five times the original scale, that is, to a scale of 440 feet to the inch. The contour lines then were used as patterns for the sawing out of thin wooden boards. These boards were built up in layers and the rough form thus obtained was plastered over with a special preparation of great durability that will bear transportation across the continent. Large numbers of photographs are being used for local details, and a special effort is being made to reproduce with fidelity the peculiar cliff sculpture which is so prominent a factor in the Yosemite landscape. Inasmuch as these sculptural forms are intimately associated with the lines of structure in the granites in which the valley lies hewn, the model promises to become an unusually fine medium for the study of these relations of form to structure. Students of geology and geography will therefore, in all likelihood, find it an object worthy of a special visit at the San Francisco Exposition. Explanatory legends will be placed on the sides of the case at various places, directing attention to the most interesting features. In order to heighten the sense of reality, small streams of water, blown to spray by atomizers, will represent the waterfalls.

THE committee appointed by the Paris Academy of Sciences to allocate the amount placed at its disposal by Prince Bonaparte is reported by *Nature* to have made the following proposals for grants during 1914: 2,000 francs to Dr. Pierre Breteau, for the continuation of his researches on the use of palladium in analysis and organic chemistry; 2,000 francs to M. Chatton, to enable him to continue his researches on the parasite Peridinians; 3,000

francs to Dr. Fr. Croze, for the purchase of a concave diffraction grating and a 16 cm. objective, to be used in work on the Zeeman phenomena in line and band spectra; 6,000 francs to Dr. Hemsalech, for the purchase of a resonance transformer and battery of condensers, to be used in his spectroscopical researches; 2,000 francs to P. Laïs, for assisting the publication of the photographic star map; 2,000 francs to M. Pellegrin, to assist him in pursuing his researches and continuing his publications concerning African fishes; 2,000 francs to Dr. Trousset, to assist him in his studies of the minor planets; 2,000 francs to M. Vigouroux, to enable him to continue his researches on silicon and its different varieties; 3,000 francs to M. Ailuaud, to assist the publication (with Dr. R. Jeannel) of the scientific results of three expeditions to eastern and central Africa; 9,000 francs divided equally between MM. Pitart, de Gironcourt and Lecointre, members of the Morocco expedition, for scientific study, organized by the Société de Géographie; 2,000 francs to M. Vasseur, for the continuation of his geological excavations in a fossil-bearing stratum in Lot-et-Garonne; 3,500 francs to Dr. Mauguin, for the continuation of his work on liquid crystals and the remarkable phenomena presented by these bodies when placed in a magnetic field; 2,000 francs to Dr. Anthony, to defray the cost of his researches on the determinism of morphological characters and the action of primary factors during evolution; 4,000 francs to M. Andoyer, to assist the publication of his new set of trigonometrical tables; 4,000 francs to M. Bénard, to enable him to continue, on a larger scale, his researches on experimental hydrodynamics; 2,000 francs to Dr. Chauvenet, for the continuation of his researches on zirconium and the complex combinations of that element; 2,000 francs to François Franck, for the chronographic study of the development of the embryo, with special examination of the rhythmic function of the heart; 2,000 francs to M. Sauvageau, for the pursuit of his studies on the marine algæ. The committee recommends these eighteen grants after considering nearly sixty applications for assistance. The amount allocated for the year is 54,500 francs.

Wide variation in the pay for the same or similar work is one of the most striking situations revealed by the investigation of teachers' salaries just completed by the U. S. Bureau of Education, under the direction of J. C. Boykin, editor of the Bureau. Public elementary school-teachers may receive $2,400 a year, as some do in New York City, or $45 a year, as in certain rural communities. Even in cities of the same class there are considerable differences in the salaries paid teachers. On the administrative side there are county superintendents with pay ranging from $115 to $4,000 per annum, and college presidents receiving salaries from $900 to $12,400. In city school systems salaries have increased steadily in recent years, particularly in the western states; and, in general, salaries in city school systems are fairly well standardized. The average salary of the superintendent of schools in cities of over 250,000 population is $7,178; the range is from $4,000 to $10,000. In the same group of cities high-school principals average $3,565 and elementary teachers $1,018. Even in the smallest cities listed, those between 5,000 and 10,000 population, salaries are fairly uniform. The maximum for superintendents in this group is $3,600 and the average $1,915; but elementary teachers show an annual average of $533, with salaries as high as $1,350 and as low as $104. It is in the colleges and universities that the widest variation prevails. The salaries of men with the rank of "professor" range from $450 to $7,500. "Professors" in some institutions receive less than "instructors" or even "assistants" in others. Salaries of deans of these institutions vary from $500 to $5,000. University teachers of subjects for which there is direct commercial demand outside receive somewhat higher salaries than those in charge of the traditional academic subjects, but the difference is less than might be expected. The highest average salaries for full professors are paid in law and civil engineering. Law claims the highest paid professorship in any subject, with one salary of $7,500; but there are professors of

physics, geology and Latin who receive $7,000. It is significant, however, that on the basis of the figures reported most college teaching, particularly in the first two years, is done by men of instructor grade with salaries of $1,000 to $1,200, or by assistants who receive on the average about $500, usually for half-time services.

UNIVERSITY AND EDUCATIONAL NEWS

DR. H. T. SUMMERSGILL, superintendent of the New Haven Hospital, has been appointed superintendent of the University Hospital in San Francisco. Dr. Summersgill has arrived to take charge of the present hospital of the University of California Medical School, and to aid in completing the plans for the new teaching hospital buildings, to erect which $615,000 has been given by various friends of the university. Dr. Winford H. Smith, superintendent of the Johns Hopkins Hospital, recently spent a month in San Francisco, coming to California as expert adviser for the plans for the new hospital.

THE six-weeks' summer session of the University of California, for 1914, enrolled 3,101 students. It is expected that next year's summer session, coming while the Panama-Pacific Exposition will be in progress in San Francisco, will much exceed this year's enrollment. The freshman class at the University of California this year, excluding special students, will number over 1,700.

THE first summer school of the George Peabody College for Teachers has just come to a close. The total enrollment reached 1,006 regular students and 99 part-time visitors, coming from 28 states, including all of the southern states. This is possibly the largest enrollment with which any summer school has started. Next year the summer school will continue for twelve weeks instead of six, thus becoming a very integral part of the year's work which is to be divided into four quarters of twelve weeks each.

FRANCIS C. LINCOLN, associate professor in the mining department of the University of Illinois, has been made the head of the Mackay School of Mines of the University of Nevada.

MR. J. E. RUSH, of the University of Wisconsin, has been made assistant professor, in charge of the departments of biology and bacteriology, at the Carnegie Technical Schools, Pittsburgh.

THE governors of the Imperial College of Science and Technology, London, have appointed Dr. A. N. Whitehead, F.R.S., to the newly constituted chair of applied mathematics, and Dr. C. G. Cullis to the professorship of economic mineralogy.

DR. T. J. JEHU, lecturer on geology at the University of St. Andrews, has been appointed Murchison regius professor of geology and mineralogy in the University of Edinburgh, in succession to Professor James Geikie.

DISCUSSION AND CORRESPONDENCE

YOUNG WHITEFISH IN LAKE SUPERIOR

THE literature on the two species of whitefish that are so important commercially in our Great Lakes (*Coregonus albus* and *C. clupeaformis*), as well as unpublished statements received by the writer from prominent ichthyologists and fish culturists, makes it appear that little, if anything, is known concerning the very young of these fish as they exist in these bodies of water. Where the young whitefish, both native and those that are planted, live and what they feed upon in their natural habitats, constitutes an important problem for ichthyologists and fish culturists. Jordan and Evermann (1902),[1] writing of *Coregonus clupeaformis*, say:

> Nothing is definitely known regarding the general distribution and habits of the young, but they are supposed to remain chiefly in the deep waters of the lake.

During August, 1913, the writer studied the fish-life of the Whitefish Point Region in Northern Michigan, as one of the investigators sent there by the University of Michigan, with funds given for the work by the Hon.

[1] "American Food and Game Fishes," page 128; published by Doubleday Page and Company.

George Shiras. While making some collections from the shallow water of Lake Superior, not far from the Vermilion Life Saving Station near Whitefish Point, eighteen little whitefish were caught, which measured from 4.9 to 9 centimeters in length, from the tip of the snout to the tip of the caudal fin. They answer very well to the description of *Coregonus clupeaformis* (Mitchill), with certain departures undoubtedly due to their immature condition; but it is possible that some or all of them may be Lake Erie whitefish (*Coregonus albus* Le Sueur) for fry of this species have been planted in Lake Superior, according to information obtained from B. W. Evermann of the Bureau of Fisheries at Washington and H. H. Marks, superintendent of the Sault Ste. Marie Fish Hatchery. It has been impossible to distinguish the two species from a study of the structure of the small fish, for the adults are thought to differ from each other only in form and color, and no evidence can be obtained that the dark, lateral bands that are thought to be characteristic of the fry of *clupeaformis,* do not disappear shortly after that stage is passed.

The food of eight of the fish examined was found to be principally entomostracans, of which the following appear to be the chief species, according to the examinations of three typical stomach contents, made by Mr. Chancey Juday, of Madison, Wisconsin: *Bosmina longirostris* O. F. Müller, *Diaptomus ashlandi* Marsh, and *Cyclops viridus* Jurine (probably var. *parcus,* Herrick). Fragments of midge larvæ and miscellaneous insects, including winged forms, and filaments of a green alga (*Ulothrix zonata*), were the other objects noted among the food.

The eighteen specimens of young whitefish were taken in several hauls made with minnow seines, drawn over the sandy bottoms where the water was less than three feet deep and through the large schools of hundreds of small fish, that were chiefly young lake herring (*Leucichthys* sp.). These were similar in size to the young whitefish associated with them, which were relatively very few in number, and superficially so like the little herrings that they could be picked from a collection only after a very careful examination of it.

Detailed descriptions of these young whitefish, their food, habitat and associates, will be given in the paper now being prepared on the fish-life of the Whitefish Point Region.

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Is the poor hatching of normal eggs due to the presence of microorganisms within the eggs?

The loss of young chicks due to the non-hatching of eggs is inestimable. Poultrymen have often said that "on an average a fifty per cent. hatch and a fifty per cent. raise was all that was generally obtained." What becomes of the other fifty per cent.? Wherein lies the cause of this heavy loss? Can it be due to the presence of microorganisms within the egg or rather to some inherent quality of the egg itself? We are aware of the fact that faulty incubation may be responsible in a large measure, but in this respect even the hen may have her troubles.

During the spring hatch we have had occasion to examine some 350 eggs, taken from both incubator and from under the hen. The eggs were those tested out as "non-fertile" or "dead in the shell." The incubation period ranged from ten days to twenty-two days. The eggs were from a flock of healthy birds and may be termed "normal" eggs.

In only one egg of the 350 eggs examined were bacteria found. The organism isolated belonged to the coli-typhi group.

From this, a preliminary report, we are of the opinion that the poor hatching quality of "normal" eggs is not directly due to the presence of microorganisms within the egg.

This work may serve to verify to a certain extent the findings of Rettger.[1]

T. D. Beckwith,
G. D. Horton

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[1] Bulletin No. 75, Storrs Agricultural Experiment Station.

HETERODERA RADICICOLA ATTACKING THE CANADA THISTLE

In addition to the large number of plants known to be attacked by *Heterodera radicicola*, the writer has recently had occasion to find it infesting a new host—the roots of the Canada thistle, *Cirsium arvense*.

On December 10, 1913, the writer noticed the first indications of the root knot, occurring on tomato plants. This crop was being grown in one of the greenhouses belonging to the Department of Horticulture. On April 28, 1914, the plants were removed on account of their unproductiveness. Many of the plants, at the time of removal, showed their entire root system infested and destroyed by this eel-worm.

On April 4, 1914, Mr. J. B. Poole, of the department of botany, called the writer's attention to nodules occurring on the roots of *Cirsium arvense*. These plants were growing in a separate greenhouse from the one in in which the tomato plants had been growing. The knots were very numerous, varying in diameter from two to ten mm. Their presence on the thistle roots, however, did not seem to interfere with the growth of this weed to any appreciable extent. A microscopic examination showed that the roots were badly infested with a nematode, and it seemed apparently to be the same species which occurred on the tomato. Cross sections of nodules showed the egg-filled bodies of female nematodes scattered throughout the cortex of the root. Specimens were sent to the Bureau of Plant Industry, and the determination verified.

The soil used in the various greenhouses was obtained from a nearby woodlot, and was probably badly infested with *Heterodera radicicola* at the time it was placed in the benches.

The fact that this organism is capable of living in the roots of *Cirsium arvense* should warrant the necessity of placing additional precautionary stress upon the eradication and destruction of this weed. Care should be exercised in treating soils before using, if the above weed should occur in any of the central or southern states, for the winters are probably not severe enough to kill the eel-worm by freezing.

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AN AVALANCHE OF ROCKS

THE Cadillac Trail on Mount Desert is one of the most picturesque features of the island. It is near Otter Creek and one enters the trail from the shore road. The trail leads by a gentle ascent to an irregular line of massive rock fragments which have fallen from some preexisting precipice farther up the mountain side. The path runs through and under and over these titanic blocks, some of which must weigh hundreds if not thousands of tons. The blocks and fragments stand at all angles. I have had no opportunity to consult any book on the geology of the island, but a hasty examination of the region leads me to believe that this avalanche of rocks must have fallen from some precipice which had been undercut by the waves when the land was below sea level, as we know the whole New England coast has been elevated since the ice sheet retreated. The glacial clays with Arctic species of Mollusks, still living in Hudson Bay, are found from Danvers, Massachusetts, east to Lubec, Maine, and beyond, and indicate a former subsidence of the coast many feet below the level of the sea. Now if this event occurred at the time of this depression the material buried beneath these rocks would be of very great interest. If some large fragment rested on the parent ledge it could be tilted sufficiently by hydraulic jacks to enable one to gather the stuff beneath, and it might reveal the shells, diatoms, foraminifera, etc., living at the time of this catastrophe, and possibly the compressed vegetation might reveal important features also. The exploration could be made at a moderate expense and the conditions could be easily restored.

EDWARD S. MORSE

SCIENTIFIC BOOKS

Problems of Genetics. By WILLIAM BATESON. Yale University Press. 1913. Pp. ix + 255, illustrated.

The Silliman lectures delivered at Yale by Professor Bateson in 1907, revised and published in 1913 under the title "Problems of Genetics," form one of the most stimulating and suggestive books for students of Evolution and Heredity which has appeared since the rediscovery of Mendel's law. Like other books by the same author, it is not designed for beginners but for actual workers in the field covered, and is no fit food for babes and sucklings. To the student familiar with current theories and lines of investigation concerning evolution the frank criticism of those theories and investigations will be especially valuable. Present knowledge is recognized to be imperfect and tentative, and while the author expresses an emphatic preference for certain views, he holds them without dogmatism or claim to finality, an attitude in scientific work which, with every advance, we need to remind ourselves of anew.

In his introduction, Bateson addresses himself to the old but unsolved question of the nature and origin of species, which he blames the discussion over natural selection for obscuring. He says: "In the enthusiasm with which evolutionary ideas were received the specificity of living things was almost forgotten. The exactitude with which the members of a species so often conform in the diagnostic, specific features passed out of account; and the scientific world by dwelling with a constant emphasis on the fact of variability, persuaded itself readily that species had after all been a mere figment of the human mind. Without presuming to declare what future research only can reveal, I anticipate that, when variation has been properly examined and the several kinds of variability have been successfully distinguished according to their respective natures, the result will render the natural definiteness of species increasingly apparent." Bateson rejects natural selection as a sufficient explanation of the origin of species, concluding that we are on safer ground "in regarding the fixity of our species as a property inherent in its own nature and constitution." He says: "As soon as it is realized how largely the phenomena of variation and stability must be an index of the internal constitution of organisms, and not mere consequences of their relations to the outer world, such phenomena acquire a new and more profound significance." In Chapters II.–IV., Bateson attempts a classification of variations along lines indicated in his "Materials" (1896). This may have been important historically in leading Bateson to his later views concerning the discontinuity of evolution, but to most readers will seem aside from the main discussion. Taking up in Chapter V. the mutation theory, he expresses the view that a new species originates in a changed "internal constitution of the organism," which with DeVries he believes to arise discontinuously. But the discontinuity, according to Bateson, is of Mendelian unit-characters or factors only. He explicitly rejects the DeVriesian idea of mutation involving a change in a whole group of charters simultaneously, and explains the peculiar genetic behavior of *Œnothera Lamarckiana* as due to hybridization.

"The facts may, I think, fairly be summarized in the statement that species are, on the whole, distinct and not intergrading, and that the distinctions between them are usually such as might be caused by the presence, absence or inter-combination of groups of Mendelian factors; but that they are so caused the evidence is not yet sufficient to prove in more than a very few instances.

"The alternative, be it explicitly stated, is not to return to the view formerly so widely held, that the distinctions between species have arisen by the accumulation of minute or insensible differences. The further we proceed with our analyses the more inadequate and untenable does that conception of evolutionary change become. If the differences between species have not come about by the addition or loss of factors one at a time, then we must suppose that the changes have been effected by even larger steps, and variations, including groups of characters, must be invoked.

"That changes of this latter order are really those by which species arise is the view

with which DeVries has now made us familiar by his writings on the mutation theory. In so far as mutations may consist in meristic changes of many kinds and in the loss of factors, it is unnecessary to repeat that we have abundant evidence of their frequent occurrence. That they may also more rarely occur by the addition of a factor we are, I think, compelled to believe, though as yet the evidence is almost entirely circumstantial rather than direct. The evidence for the occurrence of those mutations of higher order, by which new species characterized by several distinct features are created, is far less strong, and after the best study of the records which I have been able to make, I find myself unconvinced. The facts alleged appear capable of other interpretations.

"DeVries found, as is well known, that *Œnothera Lamarckiana* gives off plants unlike itself. These mutational forms are of several distinct and recognizable types which recur, and several of them breed true from their first appearance. The obvious difficulty, which in my judgment should make us unwilling at present to accept these occurrences as proof of the genesis of new species by mutation, is that we have as yet no certainty that the appearance of the new forms is not an effect of the recombination of factors, such as is to be seen in so many generations of plants derived from a cross involving many genetic elements."

The phenomenon of twin-hybrids he does not consider satisfactory evidence of *group inheritance* of characters, but to have its best explanation "in the well ascertained fact that the male and female germ-cells of the same individual may be quite different." By this is meant that the pollen and ovules of the same plant may transmit different qualities respectively.

In order to throw light on the question whether species originate discontinuously or not, Bateson discusses, in successive chapters, "Variation and Locality," "Overlapping Forms," and "Climatic Varieties," bringing together a great amount of illustrative material partly of his own collecting, partly the work of others. Special attention is given to the nearly related species of North American flickers and of warblers, which are illustrated by two beautiful colored plates. These cases have been selected because they seem to show specific differences consisting in Mendelizing unit characters. Even in these cases, however, the existence of such unit characters is inferential rather than demonstrated and actual experimental work on the crossing of wild species of birds, such as pheasants studied by Ghigi and Phillips, and pigeons studied by Whitman, though it reveals the frequent occurrence of unit character differences between one domesticated variety and an original wild species, rarely shows the existence of such differences between one wild species and another. Even granting that unit character differences occasionally occur between one wild species and another (as I believe they do), it may well be that such differences, though striking, are not the most important or essential ones. As Bateson himself says in another connection (p. 184), "It seems in the highest degree unlikely that the outward and perceptible character or characters which we recognize as differentiating the race should be the actual features which contribute effectively to that result." If an extensive survey were made of related wild species of birds or mammals, I suspect that it would be found that the discoverable differences in a majority of cases consist in quantitative differences in characters, rather than in presence and absences of striking single characters. Consider for example the genus *Mus*. The black rat, *M. rattus*, as the experiments of Morgan and Bonhote have shown, is distinguished from *M. alexandrinus* by a single unit character difference. The two cross freely and Mendelize on crossing, but without producing any new third form, so far as we know at present. The color difference between them is a very striking one, but it appears to be the only existing difference. The one might be described as a color variety of the other.

Compare now *M. alexandrinus* with *M. norvegicus*. The two are very similar in appearance. Only quantitative differences in

size and proportions of parts serve to distinguish them. Yet they are so distinct genetically that they never cross naturally and all attempts to cross them artificially have thus far failed. No one would, I think, advocate the idea that one had arisen from the other by unit character variations, such as distinguish *M. rattus* from *M. alexandrinus* or striking tame varieties of the Norway rat from the wild species. The differences in these latter cases are unquestionable, and their genetic behavior clear, but if we call forms so distinguished *species* it is evident that we are applying the term on the basis of very different phenomena from those which serve to distinguish *M. alexandrinus* from *M. norvegicus* or *M. musculus*. In these cases the observable differences are quantitative and their genetic behavior unknown. There is small reason for considering them unit character differences. It is of course possible so to regard them if one conceives of unit characters or factors in such cases as very numerous and singly with small effect, in accordance with the principle of Nilsson-Ehle. But so to conceive of a unit character is to rob it entirely of that which the theory of discontinuity in the evolution of species requires and which Bateson seeks to establish. Multiple unit characters presenting an apparently continuous series would also have no selectional value superior to that of a *truly* continuous series of variations, the conception which Bateson combats.

Bateson devotes one of the most valuable chapters of his book to the subject of adaptation, without either reaching or attempting to reach any explanation of it. Indeed he rather deplores the fact that so much attention has been devoted to the adaptation of species before we have arrived at any clear notion as to what species are or how they arise. The chief value of Bateson's discussion of this question lies in the destructive criticism which he offers of the attempted explanation of adaptation as a direct response of the organism perpetuated by heredity. He passes over the earlier discussions concerning the inheritance of acquired characters, but deals with its recent vigorous renewal by Semon, who regards heredity as analogous with memory or habit. Bateson holds that an analogy with psychic phenomena is no explanation, among other reasons because the explanation is necessarily more complicated than the thing explained. The evidence on which Semon relies to establish the inheritance of acquired characters, Bateson deals with at some length. He shows the inadequacy of the oft-cited temperature experiments with lepidoptera to show either an increased variability due to experiment or its inheritance. The case of Schubeler's wheat adapting itself automatically to the shorter season of Norway is subjected to destructive criticism, as is also the case of Brown-Sequard's guinea-pigs, so often brought forward, so often shown to be of no consequence. Special attention is given to the recent work of Kammerer at Vienna upon salamanders, on which Semon places great reliance. By keeping land salamanders in water and vice versa, Kammerer claims to have modified the structure and habits of these animals permanently, the young inheriting the acquired modifications of the parents even when restored to normal conditions, and the inherited effect increasing from generation to generation upon continuation of the experimental conditions. Bateson shows that these extensive claims are based on wholly inadequate experiments, that the author is unable or unwilling to produce specimens of the modified structures which he claims to have obtained, and that unless his observations are independently confirmed it is "easier to believe that mistakes of observation or of interpretation have been made than that any genuine transmission of acquired characters has been witnessed."

"Meanwhile there is no denying that the origin of adaptational features is a very grave difficulty. With the lapse of time since evolutionary conceptions have become a universal subject of study that difficulty has, so far as I see, been in no wise diminished. But I find nothing in the evidence recently put forward which justifies departure from the agnostic position which most of us have felt obliged to assume."

A chapter of especial interest to Americans discusses "The Causes of Genetic Variation," for the work reviewed is to a considerable extent that of American biologists, who have attempted to produce and claim to have succeeded in producing heritable variations under controlled experimental conditions. The work of Woltereck in Germany has shown, according to Bateson, that the character of the food supplied to a parthenogenetic *Daphnia* affects the structure of her immediate offspring, but the effect does not persist further into subsequent generations. Hence there is no permanent racial influence. Tower, however, in potato-beetles, and MacDougal in *Raimannia* claim to have brought about permanent racial changes, the one by altering the temperature and humidity at which the parent beetles are kept, the other by injecting certain salt solutions into the ovaries of the parent plants. Bateson points out that neither of these important results has been independently confirmed by experiment, though this has been attempted by Compton with negative results in the case of *Raimannia*. After reviewing Tower's two principal papers and pointing out a number of inconsistencies, Bateson adds

"The hesitation which I had come to feel respecting these two publications of Tower's has been, I confess, increased by the appearance of a destructive criticism by Gortner who has examined the parts of Chapter III of Tower's book in which he discusses at some length the chemistry of the pigments in *Leptinotarsa* and other animals. As Gortner has shown, this discussion, though offered with every show of confidence, exhibits such elementary ignorance, both of the special subject and of chemistry in general, that it can not be taken into serious consideration."

Regarding MacDougal's work he says, emphasizing the need of repeating the experiment with *Raimannia:*

"He [MacDougal] adds that he is making similar experiments with some twenty genera; but what is more urgently needed is repeated confirmation of the original observation. When it has been shown that this mutation can be produced with any regularity from a plant which does not otherwise produce it on normal self-fertilization, the enquiry may be profitably extended to other plants."

The net result of Bateson's discussion of the causes of genetic variation is negative. No means of controlling genetic variation has, he believes, yet been found.

A chapter dealing with The Sterility of Hybrids presents many interesting questions without answering any of them satisfactorily. Interspecific sterility is shown to be important in keeping species distinct, and it is suggested that in some cases at least it is connected with unit character inheritance, but beyond this point all is uncertainty.

In his concluding remarks, Bateson emphasizes the present partial and incomplete state of our knowledge of genetic problems and in particular of what a species really is. He expresses the conviction that it is not a mere arbitrary group of organisms, though to the systematists it can hardly be anything else. "Their business," says Bateson, "is purely that of the cataloguer, and beyond that they can not go. They will serve science best by giving names freely and by describing everything to which their successors may possibly want to refer, and generally by subdividing their material into as many species as they can induce any responsible society or journal to publish.

"As yet the genetic behavior of animals and plants has only been sampled. When the work has been done on a scale so large as to provide generalizations, we may be in a position to declare whether specific difference is or is not a physiological reality."

W. E. CASTLE

Vorträge über Deszendenztheorie. Von AUGUST WEISMANN. Dritte umgearbeitete Auflage. Jena, G. Fischer. 1913. Pp. xiv + 354, 3 pls., 137 figs. in text.

Mendel's Principles of Heredity. By W. BATESON. Cambridge, Eng., Univ. Press, and New York, G. P. Putnam's Sons. 1913. 3d Impression. Pp. xiv + 413, illustr.

These two books deal with the two most im-

portant advances which have been made in the study of evolution since the time of Darwin, namely the theories of Weismann and Mendel.

For whether one accepts or rejects these theories, no one will question their great value in stimulating research concerning evolutionary problems, the productiveness of which has been enormous in the last thirty years.

It was in the early eighties that Weismann in his essays on heredity challenged the general belief in the inheritance of acquired characters and pointed out the logical distinction between soma and germ-plasm, which despite numberless attacks still stands. Ten years later "The Germ-plasm" theory was published in its fully developed form, and after another decade of debate and study "The Evolution Theory" was published, in which Weismann attempted to make a comprehensive survey of the entire field of evolution as seen in the light of his germ-plasm theory. In the first sentence of his preface, as translated by Thomson, he says: "When a life of pleasant labor is drawing to a close, the wish naturally asserts itself to gather together the main results, and to combine them in a well-defined and harmonious picture which may be left as a legacy to succeeding generations." Succeeding generations have reason to be grateful to Weismann that he undertook thus to present his mature views. Few books on evolution since the publication of Darwin's "Origin of Species" can be read with greater pleasure or profit than this, or are likely longer to survive. To English readers it is accessible in a faithful translation made by Professor and Mrs. J. A. Thomson in their usual clear and graceful style.

The popularity of the original is shown by the fact that a second edition was called for within two years, the third and doubtless final edition being the one before us. In the second edition few changes were made, beyond the addition of a few notes, but by the time the third edition was issued (1913) Mendelism had so far developed as to call for extended review. Weismann welcomes Mendelism as a confirmation of the basic idea of his germ-plasm theory, the doctrine of determiners. Mutation he rejects as inconsistent with the view that adaptations arise gradually through the action of natural selection.

Bateson's book, first published in 1909, may be regarded as the authoritative interpretation of Mendelism. It contains a biography and three portraits of Mendel with a translation of his original papers, and also a comprehensive account of Mendelian principles as developed by the Bateson-Punnett group of workers at Cambridge University. The first edition of the book was exhausted within a few months of its publication and it was then reprinted without change. The present "third impression" was taken advantage of to add "a series of brief appendices to acquaint the reader with the nature of the principal advances made, while awaiting an opportunity of rewriting the book." The "appendices" mentioned consist of brief notices of subsequent publications, which, however, fail to give an adequate notion of their content, or of the direction which the further development of Mendelism has taken since 1909. The book is rightly and honestly called a "third impression," not a new edition. It is essentially a portrait of the Mendelism of 1909, and seeks to combine the fundamental idea in the germ-plasm theory (that of determiners) with the fundamental idea in mutation (that of the sudden origin of characters).

W. E. Castle

SPECIAL ARTICLES

A NEW METHOD FOR THE DETERMINATION OF SOIL ACIDITY [1]

Soil acidity problems are at the present time, perhaps, the most important of all soil problems confronting the farmers of Wisconsin and many other states. In studying these problems one of the most serious drawbacks has been the lack of suitable qualitative and quantitative methods for the determination of this acidity. The litmus-paper test when properly made is a fairly satisfactory qualitative test and has been our most reliable test.

[1] Publication authorized by the Director of the Wis. Expt. Station.

However, carbonic acid reacts acid to litmus, and, contrary to general belief, the reddening of litmus paper when put into carbonated water for several minutes is permanent even on drying. In testing fresh soil it is therefore necessary to keep all living plant roots away from the paper, as they may turn it red, due to the excretion of carbon dioxide. The soil water may be highly enough charged with carbon dioxide to affect the test. The moist hand must also not come in contact with the litmus paper, for that may redden the paper. When a soil is only slightly acid the litmus test is not sharp and positive and thus often causes confusion.

With a view of securing a more reliable test the writer has evolved the following zinc sulfide method. It was found that acid soils when boiled with zinc sulfide and water would liberate hydrogen sulfide, which, as is well known, can be detected very easily and positively with lead acetate paper. With this as a basis, the following method was worked out:

Ten grams of soil are placed in a 300 c.c. Erlenmeyer flask and to this is added 1 gr. calcium chloride, 0.1 gr. of zinc sulfide, and 100 c.c. water. This is thoroughly shaken and then heated over a flame. After the contents have boiled one minute, a strip of moistened lead acetate paper is placed over the mouth of the flask and the boiling continued two minutes more, when the paper is removed. If the soil is acid the paper will be darkened on the under side in proportion to the degree of acidity. If it is non-acid, no darkening will occur *if the test has been performed as just outlined.*

The calcium chloride is added to make the test more sensitive. It reacts with the comparatively insoluble soil acids and forms a small amount of hydrochloric acid which readily liberates hydrogen sulfide from zinc sulfide. The mixture is boiled one minute before putting the test paper in place in order to expel most of the carbon dioxide and also to more nearly bring all tests to the same condition before applying the paper. This test will positively detect smaller amounts of soil acids than the litmus test. The range of colors, showing degree of acidity, is large, being from white to black.

At first thought it seems possible that on boiling soils with water, some which had undergone anaerobic fermentation might give off appreciable amounts of hydrogen sulfide and thus confuse the test. On careful consideration this appears very improbable, for if the soil is alkaline any hydrogen sulfide formed in the process of fermentation will combine with the excess of bases present and is thus not given off in the test. Fresh peat and muck soils, some of which had lately been inundated, were tested and in no cases did the alkaline ones give a coloration to the test paper. The test has been applied to a considerable number of soils and also other materials of known reaction and as yet not a single objection to the test has arisen.

As a quantitative method, an effort is being made to measure the degree of acidity by titrating with standard iodine solution the hydrogen sulfide which a soil will liberate. Whether this will work with all soils has as yet not been determined. By using this test for the end point in the Veitch lime water method for acidity or lime requirements, the present Veitch method is considerably shortened and made far more accurate.

The most important part of the test, however, is the fact that it can be made approximately quantitative, and still require only very simple apparatus—such as can be carried right into the field, and require no more than ten to fifteen minutes for the determination. This will make it of great value to the extension man, field agent, etc. In fact, the farmer himself will be able to determine the lime requirements of his soil, by following very simple directions. The principle of this quantitative method depends upon the fact that for any particular class of soils the degree of acidity is closely proportional to the intensity of color produced on the paper when the test is conducted as previously outlined. The color on the test paper needs only to be compared to a standard color scale and from an accompanying table the degree of acidity or lime requirements is read off directly. This standard color

scale is now being prepared and checked up with standard soil acids made by new methods.

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Experimental efforts to retain the freshness in cut rose blooms[1]

During the spring of 1908 the Rhode Island Station had a large surplus of rose blooms from its experimental beds, and at the suggestion of Dr. H. J. Wheeler, who was then director of the station, the writer carried on over one hundred and fifty tests with solutions of various kinds of chemicals and in various concentrations, to ascertain their effect in promoting the keeping qualities of the cut blooms, whereby the average housewife could with slight trouble and expense prolong materially the period of freshness of the blooms; thus increasing largely the usefulness of the rose as a home decoration.

Most of the tests were made in May, 1908, while the final tests were completed in July, 1913.

The varieties of rose blooms used in 1908 were Brides and Bridesmaids, while the Maryland was used in 1913.

In most cases the blooms were taken immediately after being cut, and divided into uniform groups of six to eight. The stems were cut from 7 to 10 inches long, and placed in wide-mouthed flasks of 500 c.c. capacity. Ordinary water was used as the solvent in all of the tests, and a control flask was included with each lot of blooms, all of which were kept under laboratory conditions and in the original solutions for periods of from four to seven days. The number of tests of a given concentration of a chemical varied from one with the extremes to as high as three or four with some of the medium dilutions.

In the following table the degrees of concentration of the solutions are divided into the lowest used, the highest used which was not injurious, and those used which proved injurious.

PARTS IN 10,000

	Smallest Number of Parts Used	Largest Number of Parts Not Injurious	No. of Parts Used that Proved Injurious
Alcohol	—	—	50–200
Ammonium hydroxid	50	100	700–1,000
Borax	1	5	20–250
Boric acid	2	5	10
Carbolic acid	—	—	2–100
Ether	—	30	50–500
Formalin	—	—	10–200
Glycerine	—	—	50–100
Iron, solid, powdered	—	10	—
Magnesium sulfate	—	1	100
Nutrient solution[2]	5	10	—
Potassium nitrate	1	10	25
Potassium permanganate	0.2	2.5	10
Sodium carbonate	—	5	20
Sodium chlorid	1	10	100–250
Sodium nitrate	1	10	—
Sodium sulfate	—	—	2–5
Sodium sulfite	2	5	10–50
Sugar	10	1,000	—
Sulfuric acid	—	30	50–150

A mixture of one part of carbolic acid and three parts ammonium hydroxid in 10,000 did not prove injurious, while two parts carbolic acid and 50 parts ammonium hydroxid did prove injurious.

Of all the tests, a strong sugar solution, 7–10 per cent., was the only one that caused any marked freshening in the appearance of the blooms. This effect was shown by a deepening of the color of the pink varieties within a few hours after the stems were placed in the sugar solution, whereas those in water faded much sooner. However, the breaking down of the blooms and the dropping of their petals occurred at the same time in the flasks containing sugar as in those containing only water.

When clean flasks were used, the changing of the water daily, or the cutting off of the end of the stems and changing the water daily did not prolong the keeping qualities of the blooms.

F. R. Pember

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[1] Contribution 205 from the Agricultural Experiment Station of the Rhode Island State College, Kingston.

[2] Contained 20 c.c. .1 *N* $Ca(NO_3)_2$, 10 c.c. .1 *N* NH_4NO_3, 8 c.c. .1 *N* KCl, 8 c.c. .1 *N* $CaH_4(PO_4)_2$, 16 c.c. .1 *N* $MgSO_4$ and 10 c.c. .001 *N* $Fe_2(NO_3)_6$ per L.